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MORPHOLOGY AS A DYNAMIC SCIENCE[1]

By Professor EDMUND W. SINNOTT

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When a science has developed to the level where it can recognize the fundamental problems which confront it, it may be said to have passed from youth to maturity. Long ago the physical sciences were able thus to formulate their objectives, and they have made enviable progress in attaining them. Biology, on the other hand, throughout its history has moved from one major interest to another and has never seemed able to distinguish its fundamental problems from a host of minor ones, or indeed to determine whether or not there exist any strictly biological problems at all. Not many generations ago the naming and classification of the host of plant and animal species was regarded as the chief task of the biologist. This naive attitude was altered by an acceptance of the tremendous fact of evolution, which seemed to make obvious that the central problem of both botany and zoology was to write the entire phylogenetic history of the organic world, a task which commanded the allegiance of the majority of biologists for half a century.

As time went on, however, it came to be realized that the ultimate secret of a living organism will never emerge from the records of its ancestry, no matter how completely these may be deciphered. Physiology is evidently nearer than phylogeny to the ultimate problem. Stimulated by the great advances which the physical sciences had made, the attack through physiology began about a generation ago to attract many new workers and gave every promise of substantial progress. The years have found this promise amply fulfilled in our success in plotting the flow of physical and chemical change of which an organism is the seat, but the results of physiological research have tended

[1] Address of the retiring vice-president and chairman of the Section on Botanical Sciences, American Association for the Advancement of Science, Atlantic City, December 29, 1936.

to emphasize the complexity rather than the simplicity of protoplasm and have entirely failed as yet to solve the elusive problem of what an organism really is. A similar frustration has attended still another line of attack, through the science of genetics. Ever since the rediscovery of the Mendelian principles of heredity, this discipline has been enthusiastically pursued by many students who felt that here, at last, something fundamental in biology had made its appearance. The truly sensational development of the chromosome theory, with its demonstration that the genes are definite physical entities occupying constant positions in the chromosomes, has justified this early enthusiasm; but with their first major objective attained, geneticists are coming to realize that their really basic problem is not the location and transmission of genes but the mechanism by which these control the development of an organism, a question about which our ignorance is almost complete.

Although these attacks on so many widely separated fronts have not yet pierced to the center of the problem of life, they have served to clear away many obstructions and to open the road toward our chief goal, which is now just beginning to appear. Biologists are at present in the position of the early explorers of the mighty mass of the Himalayas. They have pushed in from various directions, seeking the best and most practicable routes. Many of the foothills have been climbed and a few important peaks conquered. The increasing difficulties of the terrain, once underestimated, are now recognized. Still more important, the existence of a central dominating range seems to have been established and glimpses have been gained of the very highest peak itself. The main objective of our labors is at last becoming more clearly defined.

To formulate with anything like assurance a problem which is central and fundamental for all biology, the Mount Everest of our scientific exploration, may still seem to many an act of faith rather than of sight; but within the last few decades, and recently in increasing numbers, many biologists, as well as thinkers who have approached biological problems through the physical sciences and through philosophy, are agreed in emphasizing one particular problem, one general phenomenon of life, as of primary and dominant significance. This may be stated in a word as the problem of *organization*. Living things are well termed *organisms*. The activities of their manifold structures are so integrated and coordinated that a successfully functioning whole individual develops. As to how this is accomplished very little is known. The advances of biological science have been chiefly in quite the other direction, in breaking down the organism into its constituent organs, tissues and cells, into chromosomes and genes, into protein molecules and cellulose chains, into potential differences, axial gradients and morphogenetic fields. But analysis alone, however detailed it may ultimately be made, can never lead to a complete understanding of an organism. Synthesis also is required. What it is that coordinates these various parts and processes so that an organism rather than a chaos results, what synthetic factors there may be which knit the organism together into a functioning unit, are extraordinarily difficult problems. They do not yield readily to the direct and obvious methods of attack which have usually been employed in biology and they tend to become involved in philosophical as well as strictly biological difficulties. It is probably safe to say, however, that the majority of botanists and zoologists today would admit that this problem of organization is indeed their ultimate and central concern; and that if the biological sciences have any problem peculiar to themselves and differentiating them from the physical sciences, this is the one.

My purpose in making such an excursion as this into biological fundamentals is to defend the thesis that the solution of our basic problem can be approached more simply and directly through the study of *form* than by any other means; and that morphology, far from being the hopelessly static discipline which some would have us believe, therefore touches so intimately the central problem of biology that it may still be described by Darwin's words, in a famous passage of the "Origin," as the "very soul" of natural history. Let us examine the evidence for this contention.

The correlative mechanisms by which an integrated living individual is maintained are, of course, physiological in character and are doubtless ultimately resolvable into physical and chemical processes; but the investigation from the point of view of physiology alone is usually beset by such difficulties that substantial progress on this front must wait until the necessary experimental technique is much more highly perfected than it is to-day. The coordinating and integrating capacity of protoplasm, however, is displayed not only in those correlations of function which so excite our amazement but also in the more familiar and no less remarkable correlations of growth, operative during the process of development and resulting in the production of those specific and constant shapes of organ and body which are so characteristic of living things. A fertilized egg divides this way and that in such a precise manner that an embryo with two cotyledons, a plumule and a hypocotyl, definite and specific in form, are produced. From a tiny mass of undifferentiated cells at a growing point are developed the primordia of organ after organ in a perfectly regular fashion, and each follows in its enlargement a definite

pattern of growth. In all such cases there is manifest in the clearest fashion that coordinating control of which I have spoken. Form is merely the outward and visible expression, fixed in material shape, of that inner organized equilibrium which we are seeking to understand.

A study of organization as thus expressed in form has the very great advantage that it deals with a visible and stable product, readily observed throughout its entire period of development and measurable with relative ease. The dynamic system which underlies this development of form, on the contrary, consists of a series of physical and chemical processes so complex and changing that they are much more difficult to recognize and to measure. Their product, to be sure, is less immediate to our problem than the process which forms it, but product can often be investigated where process can not. We should first examine these more tangible aspects of the phenomenon of organization, using them as a means of penetrating to the more obscure vital activities by which they are underlain.

If it be admitted that our basic problem can thus be approached most simply and directly through the door of morphology, then an investigation of the factors which determine organic form assumes a major place in biological science. That this importance is coming to be generally recognized is evident in the diversity of directions from which developmental problems in plants and animals are now being attacked. Physiology has always regarded correlative development as an integral part of its domain, but in recent years this subject has assumed a steadily growing importance, as witness the intensive researches on hormones, organizers, metabolic gradients and morphogenetic fields. Genetics is now increasingly concerned with an attempt to discover how genes control development and thus produce the traits by which they are recognized. Ecological attack upon the problem of changes in form through environmental factors has been intensified by discoveries in various fields. Even physicists and chemists have been intrigued by developmental problems and have made important contributions toward their solution. This field of investigation—call it experimental morphology, causal morphology or morphogenesis—is thus drawing to itself some of the best thought and skill of the biological sciences and promises soon to assume a position of major interest and activity.

In this diversified attack upon the problem of the causes of the coordinated developmental processes which result in the production of organic form, only a relatively minor part, strangely enough, has been played by those biologists who might have been expected to be more interested in it than any one else—the morphologists themselves. With important exceptions, those botanists and zoologists whose primary concern has been with the form and structure of living things have contented themselves with the static and descriptive aspects of their science rather than with its dynamic and developmental side. The reason for this one-sided emphasis in morphology is evidently a historical one. The form of organisms has always fascinated biologists. Its constancy in each species, its almost infinite diversity and the existence of underlying similarities in form between groups of organisms have persistently demanded an explanation. Long delayed though this was, it seemed at last to have been completely and triumphantly provided by the theory of evolution. What could be more obvious than that all this diversity of form was the result of evolutionary divergence? What more certain than that structural homology was due to common ancestry? Under the tremendous impact of this new idea it was inevitable that students of organic form should regard as their primary task a careful description of the external and internal structure of plants and animals so that by diligent comparison of a wide range of types the evolutionary history of the organic world could be reconstructed. In the period of its greatest expansion morphology thus became preoccupied with phylogeny to the exclusion of almost everything else, and this primary interest has largely persisted to the present time.

Such preoccupation is to be explained not only by the importance of the phylogenetic task itself but by the inherent attractiveness of such problems as these. The piecing together of evidence from many sources, the reconstruction of divergent lines of evolutionary descent within a group of organisms, and the recognition of homologies between apparently diverse structures excites the same sort of interest as does a jig-saw puzzle or a detective story and appeals to the primitive human urge to bring order out of chaos. No one who has ever tried to solve a phylogenetic problem can fail to recognize the peculiar fascination which it possesses for its votaries.

With all these influences at work it is therefore not surprising that the purely descriptive and historical phases of their work have attracted the chief attention of most of those whose major interest is with the study of organic form. The results of this study have been of very great significance in the development of biology, and the writer has no wish to disparage them in any way or to belittle the contribution which they have made and will continue to make toward our understanding of living things. Nevertheless, if the argument developed in the present paper is sound, the dynamic aspect of the problem of form is of far greater ultimate significance than its descriptive side alone. Morphology should concern itself with causes

as well as with results, and should not abandon this most promising, though most difficult, part of its territory to be explored by physiology, genetics, biochemistry and other sister sciences whose main interests lie elsewhere. If it is at this spot where the chief treasure is hidden, the cooperation of all is surely to be welcomed in bringing it to light, but those who first staked out a claim here should lead in the search and be sinking the deepest shaft.

To all this it may be objected that names are unimportant; that whether those who attack the dynamic aspect of form call themselves morphologists or cytologists or biophysicists is quite immaterial, for no morphological caste or guild can claim precedence for itself here. Of course this is true, but as a practical matter it should not be forgotten that the material which presents itself to the student of morphogenesis is complex and requires a rather special knowledge on the part of the investigator if he is to be safe from error and waste of effort. An outsider is notoriously prone to make absurd mistakes if he works in a field which is not his own by experience and training, and nowhere is this more true than in problems involving the data of morphology. One who is well trained in this field has a very real advantage in morphogenetic studies.

But the morphologist may object again that by temperament and training he is unfit to undertake problems involving the dynamic side of his subject, since these require an approach through experiment and the methods of the physical sciences, with which he is often unfamiliar and unsympathetic. As he can not thus be of real service here, he may ask, why not leave him in the ivory tower of his phylogenies and his life histories and turn over to the physiologists and their allies, fortified by a little better morphological training, the whole troublesome task of determining the causes of form?

Such a defeatist attitude, it seems to me, is based on the erroneous assumption, often made by both morphologists and non-morphologists, that the only way to attack the problems of morphogenesis is by experiment, involving almost immediately the techniques of the physical sciences. No one, of course, questions the great importance of the experimental method or the desirability of resolving as promptly as possible the problems of development into the simpler ones of physics and chemistry; but as a matter of sober fact, most of these problems are not yet in a position where they can profitably be attacked in this manner at all. Before we can intelligently set up experiments to determine the integrating and coordinating growth processes which control development and produce specific forms, we must first obtain precise descriptive information as to *exactly how development proceeds.* Furthermore, in most cases where as the result of experiment a difference of form or structure has been produced, it is of the utmost importance to analyze in morphological terms the exact changes involved. Long before normal development, or experimentally produced changes in it, can be expressed in physical or chemical terms, they must be expressed in morphological terms. The first step backward from the visible end result of a developmental process toward the ultimate inducing cause—be this gene, hormone or radiation—must be a more refined *description* of this result and of the visible steps which lead up to it. This is obviously a job for the morphologist. An enormous amount of spade-work of this sort needs to be done in almost every morphogenetic problem, for our knowledge of the exact steps in the development of most organs, in terms of cells, tissues and precise visible changes, is still shockingly meager. In our haste to interpret results in ultimate terms we have too often failed to find out exactly what these results really are. The chief service which the descriptive morphologist can do for the experimental morphologist is to provide just this sort of information. No one can do it as well as he. I believe that there is no other task confronting him which is so important.

But it is not only a descriptive knowledge of development as expressed in words that the student of morphogenesis requires. In one important particular the morphologist must change his usual technique if he is to make it serve the dynamic aspect of his science: *he must present his results in quantitative terms.* Only thus can they yield themselves to precise analysis and to interpretation in terms of the physical sciences, and only thus can they serve as a means for the discovery of new facts and relationships. To the scalpel and forceps, the microtome and the microscope, the morphologist must add the ruler and the scale as part of his equipment if he is to make his data serviceable to morphogenetic science.

An example or two will illustrate the essential part which quantitatively descriptive morphology can play in developmental and morphogenetic problems.

The coleoptile of the oat has long been an important organ for the study of the effects of plant hormones on development. Its growth and angle of bending have been measured in many experiments, but not until recently was its developmental history carefully studied in terms of internal structure. Avery and Burkholder have now determined the distribution and duration of cell division within it and have measured the changes in cell size in all its tissues and in all stages from seed germination until it reaches maximum growth. This was a morphological task, but it has provided the necessary basis for any thorough-going analysis of the precise effects of hormones on the development of this organ.

In my own laboratory we have been studying the

genetic basis of shape differences in the fruits of the Cucurbitaceae. These characters can be described by the patterns and shape indices of the mature fruits, but such tell only part of the story. It is essential to learn the developmental history of each type if we are to find what the genes actually control here. When length and width are measured at successive stages from ovary primordium to ripe fruit it is found that they grow at different rates, so that the fruit changes in shape somewhat during its development. The relative growth rate is consistently different in different races. In the Hercules club, length grows faster than width, so that the fruit becomes progressively more elongate. In the bottle gourd, on the other hand, width grows faster than length. Within a given race, however, this relationship is so unvarying that it may be expressed by a simple value or constant and thus used to describe very precisely the most important aspect of a fruit-shape difference. This constant relative growth rate segregates in inheritance and seems to be what the genes governing shape primarily control. It thus constitutes an important step into that unknown territory between the gene and the visible shape which this determines. The existence of such a constant relationship as this in the midst of developmental diversity and change could not have been recognized without a careful descriptive study of the entire history of the growing fruit, expressing its results not only in words but in measurements.

Such examples could be multiplied almost indefinitely, and from work with animals as well as with plants. The whole domain of developmental morphology, illuminated by the ideas and view-point of morphogenetic research and attacked by quantitative as well as qualitative methods thus offers a wide field for fruitful investigation. Let no one disparage such studies as "merely descriptive." Description must precede explanation, and in the combined attack on the problem of organization the morphologist should be a leader, not a follower. His is the task of the pioneer entering a wilderness of facts, which must be explored and cleared up before those who follow in his steps can practice their arts of greater refinement and precision.

For the welfare of biology as a whole, therefore, it is my plea that those who have been trained in the rigorous disciplines of morphology may turn in increasing numbers to the more dynamic aspects of their subject. Especially let us hope that those younger botanists and zoologists who choose to devote themselves to the problems of organic form may realize that these can not be set apart as a static compartment of biological thought but must touch and illuminate the whole. May they help to resolve for us this fundamental paradox: that protoplasm, itself liquid, formless and flowing, inevitably builds those formed and coordinated structures of cell, organ and body in which it is housed. If dynamic morphology can come to the center of this problem, it will have brought us close to the ultimate secret of life itself.

OBITUARY

STANLEY R. BENEDICT

THE death of Stanley Rossiter Benedict on the night of December 21 was a grievous shock to his friends and colleagues. He was only fifty-two years of age, and while he had suffered some physical disabilities in recent years he seemed to his friends to be in the prime of useful life until about a week before his untimely end.

Benedict's claims to distinction are of a very substantial order. As professor of biological chemistry in the Cornell University Medical College, he was a teacher of wide repute, who added much to the dignity of a young and growing department, where many workers of both sexes obtained not only knowledge but standards of scientific integrity which served them well in later life. His collaborators make up a lengthy list, and in addition to the younger workers his long association with Emil Osterberg, who survives him, is happily commemorated in many joint publications. Like all generous men Benedict was only intensely pleased with the successes that came to his former pupils. His early training had been in part at New Haven, and in most respects he was a true disciple of the Chittenden-Mendel tradition of physiological or metabolic chemistry. He possessed in addition masterly skill in analytical chemistry, a sound appreciation of physiology and considerable knowledge and ready understanding of the problems of structure that organic chemistry was presenting to the developing science of biochemistry. His attitude to the purely physical side of his subject may probably be described as receptive and sympathetic rather than enthusiastic.

His skill as an analyst can only be compared with that of Folin, with whom it must be confessed he was frequently in spirited argument, which only served to cement the underlying friendship of the two men, who really had much in common. Benedict's researches on the estimation of sugars, creatine, creatinine, purines, uric acid, phenols, sulfur, glutathione, ergothioneine and many other substances, by both macro and micro methods have become part of every biochemist's training. But he was not content with analysis for its own

sake and not infrequently made important discoveries of new substances whose presence had been indicated by the use of his precise analytical methods. Thus, for example, he isolated the interesting sulfur-containing compound "thiasine" from blood corpuscles and later identified this substance with ergothioneine, which had hitherto only been encountered in ergot. In similar fashion he was led to the isolation from blood of a beautifully crystalline compound of uric acid and ribose, and the guess may be hazarded that this totally novel discovery gave him as much personal satisfaction as any of his other investigations. Benedict's work in the field of metabolism covered an extensive range. Many fruitful investigations were carried out on glycosurias of various types and on the creatine-creatinine problems, while his work on the relation of the kidney to ammonia formation and excretion was stimulating and distinctly upsetting to the currently accepted doctrines. For a long time Benedict was associated with the Memorial Hospital in New York City and in conjunction with his old pupil Sugiura was responsible for a vast amount of useful information concerning the influence of various chemical and other agents on the growth of tumors.

In 1920 it became necessary for various reasons to find a new home and new managing editor for the *Journal of Biological Chemistry*. The home was provided through the generosity of Cornell University Medical College, and in spite of considerable hesitation, Stanley Benedict, who had long been one of the journal's most distinguished contributors, was at last persuaded to accept the managing editorship. The personal sacrifice involved was immense, but until the day of his death he gave of the very best that was in him to further the interests of the journal and the science that it represented. In this labor of love he was ably supported by Miss Smalley and her devoted associates. Probably few people except editors know much of the never-ending grind and human difficulties entailed in the successful editing of a scientific journal. The *Journal of Biological Chemistry* has indeed been fortunate in this respect, and Benedict has set a standard that will not easily be surpassed. He was an editor who really edited and was not content to pass for publication indifferent material simply because it happened to originate from individuals or institutions of standing. To some extent he had Samuel Johnson's dislike of impairing the clarity of expression of his views or judgments by surrounding them with a sugar coating of innocuous words. He was direct, forceful, tenacious in argument, but absolutely unswayed in his judgments by any consideration other than the facts as he saw them. His intimates knew that under a somewhat stern exterior he was the kindliest and friendliest of men, with a keen sense of humor and a very charming smile. Indeed, Benedict was always susceptible to a little innocent raillery and would go more than half way to meet a joke. On one occasion in early days when as editor he had decided, against the views of at least one of his colleagues, to amputate a good many of the final "e's" that terminate the names of so many biochemical compounds, a solemn request as to whether he proposed similarly to abolish the final "e" in the name of his patronymic liqueur "Benedictine" brought an immediate suspension of operations.

Few men had less desire for honors than Benedict. He was a member of the National Academy of Sciences, a past president of the Society of Biological Chemists and received many other notable distinctions, all of which he bore with a refreshing lightness.

He was born in Cincinnati on March 17, 1884, son of Professor Wayland Richardson and Anne Kendrick Benedict. His father was professor of philosophy and psychology at the University of Cincinnati. His maternal grandfather was A. C. Kendrick, professor of Greek, Hebrew and Sanskrit at the University of Rochester and a member of the American committee for the revision of the King James version of the Bible. He graduated from the Universities of Cincinnati and of Yale, and taught at Syracuse University and Columbia University before going to Cornell University. In 1913 he married Ruth Fulton, of Norwich, N. Y., a well-known ethnologist, who survives him. He had also three sisters, each of whom has achieved professional distinction.

Benedict's memory will long be cherished by his university, by the journal he loved and served so devotedly, and by his many colleagues and friends, who found in him a source of both stimulation and good fellowship.

H. D. DAKIN

GRAFTON ELLIOT SMITH

SIR GRAFTON ELLIOT SMITH was so well known and had so many friends and colleagues in this country that some comment on his life and personality and his contributions to science may be acceptable, even though SCIENCE does not usually print obituary notices of foreign men of science.

With regard to his childhood and youth, not long ago he told one of his recent students that when he was a very young boy he began to collect fossil ferns, which were found near his birthplace at Grafton, New South Wales, Australia. When about fourteen years old he attended an evening lecture on the brain, in which the lecturer described the complexity of the convolutions of the human brain and added that many of these convolutions did not even have names or definite boun-

daries. He resolved not only to give them names but to discover their functions.

After studying anatomy and medicine at the University of Sydney, N. S. W., he took up the practice of medicine in that province. At that time, as he said, his youth protected him from having many patients, so he was free to collect the wonderfully primitive egg-laying and pouched mammals of Australia, to dissect them and to study the construction of their brains. His observations in this field were both original and important and led to the publication, chiefly in the leading English scientific journals, of a series of papers on the structure and evolution of the brain of the duck-bill (*Ornithorhynchus*) and other primitive or archaic mammals, including the South American edentates. In many cases he succeeded in showing that there was a definite relationship between the peculiar mode of life of the animal and the development of certain parts of the brain.

He left Australia to accept a fellowship at the University of Cambridge, and after two years was called to Cairo, Egypt, to become professor of anatomy at the Government Medical School. It was during this period that he extended his studies to the brains of primates, beginning with the lowly lemurs of Madagascar and Africa and working upward through the series to the "spectral tarsier" of Borneo, the marmoset and other monkeys of South America, the macaques and other Old World monkeys, finally to the anthropoid apes and man.

As a result of these and later studies, he gained an increasingly penetrating insight, first, into the progressive complications of the main regions of the brain during the course of evolution from fish to man and, second, into the relations between special areas and groups of brain cells on the one hand and particular responses on the other.

Since the elder Huxley was always his ideal, he spared no pains to translate even the most technical results into diagrams and simple descriptions which people of intelligence could readily comprehend, and no one who was ever privileged to hear him lecture would be likely to forget the experience.

At Cairo, his friend Professor (later Sir) Flinders Petrie soon plied him with delightful anatomical puzzles presented by the mummified remains from the tombs of the ancient Egyptians. Here his studies of the skulls and skeletons soon showed that the earlier Egyptians, who had narrow skulls, had been invaded at a certain period by a round-skulled people of Asiatic and more or less Armenoid appearance. The Egyptian embalmers took care to preserve the heart, kidneys and other organs separately and then to replace them within the body, and Professor Elliot Smith found that after certain restorative measures had been applied these ancient tissues could be sectioned, stained and studied under the microscope. This proceeding enabled him to hold post-mortem examinations on the bodies of persons who had been dead for several millennia. Moreover, he found that in certain cases the embalmers had made serious mistakes, transposing or omitting certain organs and presumably causing some rather embarrassing situations in the world of shades.

Almost in spite of himself, Elliot Smith extended his interests from purely anatomical fields to widening circles of cultural anthropology and it was indeed on this side of his career that he was best known to the world at large.

From Cairo he returned to England to occupy the chair of anatomy at the University of Manchester. For the past fifteen years he was head of the department of anatomy of University College, London, and there continued his work on the comparative anatomy of the brain but found time to write a whole series of books and papers in defense of his theory that civilization had originated in Egypt and had spread thence through Asia Minor and Persia to India, China and eventually to Central and South America. On this last topic he encountered the unanimous opposition of leading American archeologists and ethnologists, who maintained that in the case of at least the more advanced native American civilizations there is the most detailed and cumulative evidence of their having attained their peculiar cultural characteristics exclusively in the Americas, so that such resemblances as they do show to certain Old World cultures, as, for example, in the use of the truncated pyramid, the idea of the feathered serpent, the practice of mummification and the like, have more probably arisen independently in the New and Old Worlds through parallel or convergent development, that is, through similar reactions to similar situations, originating in the basic identity of human desires, motives and mentality the world over.

Elliot Smith took an active part in the study and discussion of the fossil Piltdown skull, the *Pithecanthropus* of Java, the *Sinanthropus* of China, as well as the *Australopithecus* of South Africa, the "Lady of Lloyds," and other fossil human crania. In these fields he sometimes contested the conclusions of his life-long friends, Sir Arthur Keith and Dr. Eugen Dubois, the discoverer of the *Pithecanthropus,* but the resulting discussions were invariably important, especially as aids in the sifting out of errors due partly to incomplete preservation of material.

Elliot Smith drew to his laboratories a notable assemblage of students and junior colleagues, among

whom may be mentioned: Davidson Black, the describer of the Peiping skull (*Sinanthropus*); Raymond Dart, describer of the *Australopithecus* of Taungs, South Africa; Wingate Todd, the well-known anatomist of Cleveland, Ohio; Frederic Wood-Jones, author of "Man's Place among the Mammals"; W. E. Le Gros Clark, author of "Early Forerunners of Man"; H. H. Woollard, author of monographs on the brains of *Tarsius* and other primates; H. A. Harris, now professor of anatomy at Cambridge University; Joseph Shellshear, formerly professor of anatomy at Hongkong University; D. E. Derry, of the Government Medical School at Cairo, Egypt; W. J. Perry, of the Section of Cultural Anthropology, University of London; John Beattie, Conservator of the Museum of the Royal College of Surgeons, London; Una Fielding, who, it is to be hoped, will complete and publish Elliot Smith's text-book of anatomy, and many others.

It is well known among his students and associates that Elliot Smith freely gave stimulating suggestions and ideas to those around him and gladly assisted them in the testing and development of both his own and others' problems. He was also admired and influential among English zoologists and vertebrate paleontologists, who followed his work, especially on the evolution of the brain, with keen interest. His work in cultural anthropology, although far more widely known, will not, in the opinion of many of his colleagues, constitute so enduring a monument to his memory as will his studies on the comparative physiology and evolution of the human brain.

On the side of public service, Elliot Smith was for many years the trusted adviser of the Rockefeller Foundation, which sent him on special missions to the United States, England, Egypt, China and other countries. It was doubtless due partly to his advice that the Rockefeller Foundation gave its powerful backing to the sciences of anatomy and zoology in all these countries.

In personal appearance, at least in his later years, Sir Grafton Elliot Smith somehow suggested the best portraits of George Washington. In his lectures there was a certain polish and grace joined to an intense sincerity and becoming modesty. He loved to tell jokes on himself, of which he had a choice collection. To him science knew no national boundaries and both as a man and a scientist his genial influence was felt in many countries, especially Australia, England, Canada, Holland, the United States and China.

WILLIAM K. GREGORY

AMERICAN MUSEUM OF
NATURAL HISTORY

RECENT DEATHS AND MEMORIALS

DR. JULIUS O. STIEGLITZ, professor of chemistry and chairman of the department at the University of Chicago, died on January 10, in his seventieth year.

DR. FREDERICK V. COVILLE, botanist of the U. S. Department of Agriculture, with which he had been connected since 1888, died on January 9. He was sixty-nine years old.

DR. DAVID FRASER FRASER-HARRIS, secretary of the faculty of medicine of the University of Birmingham, England, and formerly professor of physiology at Dalhousie University, Halifax, N. S., died on January 3 at the age of sixty-nine years.

DR. R. F. C. LEITH, until his retirement in 1919 with the title emeritus professor of pathology and bacteriology at the University of Birmingham, died on December 14 at the age of eighty-two years.

LIEUTENANT COLONEL SIR DAVID SEMPLE, specialist in tropical disease, died on January 8. He was first director of the Pasteur Institute in India. On his retirement in 1905 he accepted service under the Government of India to organize the Central Research Institute of India. He was eighty years old.

Nature announces the following deaths: Sir Herbert Jackson, formerly director of the British Scientific Instrument Research Association, on December 10, aged seventy-three years, and Dr. A. A. Robb, author of works on aspects of relativity, on December 14, aged sixty-three years.

MEMORIAL exercises in honor of the late Julius Arthur Nieuwland were held at the University of Notre Dame on Sunday, January 10. Religious exercises took place in the morning. In the afternoon the program was presided over by the Rev. Francis J. Wenninger, dean of the College of Science, University of Notre Dame. Speakers taking part in the ceremonies and their subjects were as follows: "Father Nieuwland the Botanist," Dr. Marcus Ward Lyon, Jr., formerly assistant curator, U. S. National Museum; "Father Nieuwland the Chemist," William Stansfield Calcott, director, Jackson Laboratories, E. I. du Pont de Nemours and Company; "The Energy Balance of Star Systems," Dr. Arthur Haas, professor of physics, University of Notre Dame; "The Vanishing Floras of Northeastern America," Brother Marie Victorin, professor of botany, University of Montreal; "A Relativistic Theory of Atomic Structure," Dr. George David Birkhoff, Perkins professor of mathematics, Harvard University, and "Large Molecules in Science and Life," Dr. Hugh Stott Taylor, David B. Jones professor of chemistry, Princeton University.

SCIENTIFIC EVENTS

THE SOIL CONSERVATION SERVICE

THE annual report to the Secretary of Agriculture of H. H. Bennett, chief of the Soil Conservation Service, has been made public. He points out that the effectiveness of soil and water conservation at flood sources in minimizing floods is substantiated by several established facts:

1. Flood loads are due in large part to rapid surface run-off of rainfall or melting snow and the quick concentration of this water in stream channels.
2. The upland soils of a watershed constitute a storage reservoir capable of absorbing or retaining enough water to prevent, or at least greatly reduce, critical flood crests in the lower drainageways.
3. Proved and adaptable procedures to hold water in the soil are now available.

Mr. Bennett states that at the close of the year there was a wide-spread and growing conviction that the solution of the nation's flood problem lies in a coordinated watershed program of prevention and control in which the upstream farmer will reinforce with soil and water-saving practices the downstream fortifications of the engineer at critical areas of great danger. The former would prevent floods as far as possible and the latter would control critical flood crests when they do arise. To show the effectiveness of soil- and water-conserving practices in the alleviation of flood and drought conditions, Dr. Bennett cites data obtained by the Soil Conservation Service at its erosion experiment stations throughout the Great Plains.

The field activities of the service were considerably enlarged in the fiscal year. The number of demonstration projects was increased from 47 to 143 and the area of privately-owned land under cooperative agreement increased from approximately 4,000,000 to 7,000,000 acres. By July 1, 1936, nearly 18,000 farmers had signed voluntary agreements to cooperate with the service.

From a technical standpoint, the demonstration program remained unchanged during the year. It continued the introduction of such beneficial farming practises as strip cropping, contour tillage and contour furrowing; the construction of terraces, check dams and water-spreading dikes; woodland and gully plantings, and the retirement of steep slopes and badly eroded areas from cultivation.

More than 430,000 acres in the soil conservation demonstration projects, including the CCC camp areas, have been strip-cropped. Almost 200,000 acres have been contour-furrowed, and more than 900,000 acres have been tilled on the contour. Almost 38,000 miles of terraces, together with more than 200,000 terrace outlet structures, have been completed. Approximately 900,000 small dams have been built to check the run-off of rainfall and the spread of gullies.

In addition to these demonstration activities on private land areas, the service is conducting erosion control work on four large areas of federal and public land. These projects are the Navajo Indian Reservation of 17,000,000 acres in New Mexico and Arizona; the upper Gila River watershed, including the San Pedro and Santa Cruz tributaries, comprising 13,900,000 acres in New Mexico and Arizona; the Rio Grande watershed above Elephant Butte Reservoir, embracing 14,300,000 acres in New Mexico, and the entire Shoshone Indian Reservation of 2,400,000 acres in Wyoming.

RESEARCH PROGRAM OF THE FOOD AND DRUG ADMINISTRATION

AT the request of Secretary of Agriculture Wallace, the National Academy of Sciences, through its president, Dr. Frank R. Lillie, has appointed a committee for the purpose of reviewing the research program on the toxicity of lead and arsenic now under way in the Food and Drug Administration. The committee appointed by Dr. Lillie, that recently held its first meeting in Washington, consists of Professor A. J. Carlson, of the University of Chicago, *chairman;* Professor C. K. Drinker, of Harvard University; Dr. Ludvig Hektoen, McCormick Institute for Infectious Diseases and chairman of the National Research Council; Professor H. C. Sherman, of Columbia University, and Professor Torald Sollmann, of Western Reserve University School of Medicine.

The problem of the degree of toxicity of lead and arsenic occurring in the form of spray residues on fruits and vegetables has long been a troublesome one. Fruit and vegetable growers are obliged to use lead arsenate sprays to guard their crops against insect pests. Such sprays are useless unless they are sufficiently poisonous to destroy the insects. The residues remaining, if in sufficient quantity, are also dangerous to consumers. There is no difference of opinion among scientific men as to the poisonous character of both lead and arsenic. Authorities differ only upon the amounts of these poisons which may be consumed without damage to health.

The Food and Drug Administration of the Department of Agriculture has for years been carrying on a campaign under the Food and Drugs Act to remove from the market consignments of fruits and vegetables bearing what are considered dangerous amounts of poisonous residues. Other bureaus of the department have developed washing methods and appliances for

the removal of excess residues before the products are shipped, and these are in very general use, particularly in the apple industry. The present tolerances were adopted on the basis of advice given by a committee of toxicologists called together for consultation about ten years ago. That committee, in recommending the tentative tolerances which are essentially those now in effect, recommended further researches to fill out some of the gaps in scientific knowledge of the subject and determine more conclusively than has heretofore been possible at what figure permanent tolerances for lead and arsenic should be set to guarantee public health protection.

With an increase in appropriation for the enforcement of the Food and Drugs Act granted to the department two years ago by Congress, the Food and Drug Administration organized a Division of Pharmacology under the leadership of Dr. Edwin E. Nelson, who was furloughed by the University of Michigan, for the purpose of selecting competent personnel and formulating a comprehensive program of research on the toxicity of lead and arsenic as well as on other problems. Having completed the organization of the division, Dr. Nelson returned to the University of Michigan on October 1, where he is now professor of pharmacology. He continues to assist the division, however, in a consulting capacity. Dr. Nelson was succeeded as chief of the division by Dr. Herbert O. Calvery, biochemist. The division consists of twelve technically trained men, including eight biochemists and nutritionists, three pharmacologists and one pathologist.

As the first and most important subject for consideration by the new division, Dr. Nelson and Dr. Calvery outlined the research project for the study of the toxicity of lead and arsenic, which will be continued over a period of some years, with the objective of giving a scientific answer to the question as to what are safe tolerances for these poisonous substances.

THE ANNUAL MEETING OF THE GEOLOGICAL SOCIETY OF AMERICA

THE forty-ninth annual meeting of the Geological Society of America was held at the Netherland Plaza Hotel, Cincinnati, from December 29 to 31.

Nearly six hundred persons registered for the meeting. The scientific program carried one hundred and fifteen titles, and the programs of the associated societies, the Paleontological Society and the Mineralogical Society of America, were also crowded.

The address of the retiring president, Dr. Walter C. Mendenhall, entitled "Outline of the Evolution and Present Status of Geology in North America," was delivered on the evening of December 29.

The annual dinner was held on the evening of the thirtieth. The ninth award of the Penrose Medal of the Geological Society of America was made at this time, the recipient being Professor Arthur Philemon Coleman, professor emeritus of the University of Toronto. The presentation address was made by Professor George D. Louderback, chairman of the Medal Award Committee.

On the afternoon of December 30, Dr. Isaiah Bowman made a radio address over Station WLW, entitled "Geology in the Evolution of Culture."

The officers of the society for the year 1937, elected at the annual meeting, are as follows:

President, Charles Palache
Past-President, Walter C. Mendenhall
Vice-Presidents, W. O. Hotchkiss, Charles Camsell, G. D. Harris, W. S. Bayley
Secretary, Charles P. Berkey
Treasurer, Edward B. Mathews
Councilors, Hoyt S. Gale, Chester R. Longwell, M. M. Leighton, Joseph T. Singewald, Jr., Walter H. Bucher, Russell S. Knappen, E. L. Bruce, Joseph Stanley-Brown, G. F. Loughlin

The following geologists were elected foreign correspondents:

Lucien Cayeux, Paris, France
Arthur Holmes, Durham, England
Louis de Launay, Paris, France

The following is the list of newly elected fellows:

John Emery Adams, Midland, Texas
John Hodgdon Bradley, Jr., Los Angeles, California
Carl Colton Branson, Providence, Rhode Island
Roland Wilbur Brown, Washington, D. C.
Edwin Harris Colbert, New York, N. Y.
George Vibert Douglas, Halifax, Nova Scotia
Lloyd Wellington Fisher, Lewiston, Maine
Paul Pavel Goudkoff, Los Angeles, California
Philip Krieger, New York, N. Y.
William Christian Krumbein, Chicago, Illinois
Ralph Maxwell Leggette, Jamaica, New York
Evans Blackmore Mayo, Bishop, California
Simeon William Muller, Stanford University, California
William Thomas Nightingale, Rock Springs, Wyoming
Henry Staats Sharp, New York, N. Y.
Victor Timothy Stringfield, Washington, D. C.
Charles Vernon Theis, Albuquerque, New Mexico
Norman Edward Weisbord, Sumatra, Netherland East Indies
Alice Evelyn Wilson, Ottawa, Canada

THE AWARD OF THE PERKIN MEDAL OF THE SOCIETY OF CHEMICAL INDUSTRY

THE William H. Perkin Medal of the American Section of the Society of Chemical Industry was presented to Thomas Midgley, Jr., vice-president of the Ethyl Gasoline Corporation, at a meeting on January 8 at the New York Chemical Club.

The medal was presented to Mr. Midgley "for dis-

tinguished work in applied chemistry, including the development of antiknock motor fuels and safe refrigerants." The presentation speech was made by Professor Marston T. Bogert, of Columbia University, past president of the American Chemical Society, of which Mr. Midgley is now chairman of the board of directors. Dr. Robert E. Wilson, vice-chairman of the Pan-American Petroleum and Transport Company, spoke on "The Life and Accomplishments of the Medallist." Mr. Midgley gave an account of his work in his address of acceptance, which was entitled: "From the Periodic Table to Production," in the course of which he paid the following tribute to his associates:

Charles F. Kettering was a primary factor in solving the two problems for which I am rewarded. Without his guiding genius, faith, patience and financial support it is quite likely that neither Ethyl gasoline nor the Freon refrigerants would be in existence to-day. The assistance of T. A. Boyd and Carroll A. Hochwalt in the development which led to the discovery of the utility of tetraethyl lead in motor fuels can not be overemphasized. Albert A. Henne deserves fully as much credit as I do for developing the organic fluorides as refrigerants.

After graduating from the Sibley College of Mechanical Engineering of Cornell University, Mr. Midgley engaged in private research in tires until 1914, when he became superintendent of the Midgley Tire and Rubber Company, Lancaster, Ohio. Since 1920 he has worked in the General Motors laboratories in association with Mr. Kettering. He is vice-president of Kinetic Chemicals, Inc., and has contributed widely to the knowledge of the properties of natural and synthetic rubbers and methods of making better synthetic rubbers.

He is a fellow of the American Association for the Advancement of Science and a member of the American Institute of Chemical Engineers, the Society of Automotive Engineers, the Society of Military Engineers, the American Public Health Association, the Association of Cornell Engineers, and Sigma Xi, Phi Kappa Phi and Atmos. He has received both the Longstreth and Nichols Medals in recognition of his scientific work, and is the author of nearly fifty technical papers and the holder of more than forty patents.

Former Perkin medalists besides Sir William Perkin were: J. B. F. Herreshoff, Arno Behr, E. G. Acheson, Charles M. Hall, Herman Frasch, James Gailey, John W. Hyatt, Edward Weston, L. H. Baekeland, Ernest Twitchell, A. J. Rossi, F. G. Cottrell, Charles F. Chandler, Willis R. Whitney, William M. Burton, Milton C. Whitaker, Frederick M. Becket, Hugh K. Moore, R. B. Moore, John E. Teeple, Irving Langmuir, E. C. Sullivan, Herbert H. Dow, Arthur D. Little, C. F. Burgess, George Oenslager, G. O. Curme, Jr., Colin G. Fink, Warren K. Lewis.

Dr. Vail, chairman of the American Section of the Society of Chemical Industry, presided at the meeting, in which the New York Section of the American Chemical Society participated. The event was preceded by a dinner in honor of Mr. Midgley.

SCIENTIFIC NOTES AND NEWS

DR. RICHARD P. STRONG, professor of tropical medicine, Harvard Medical School, has been elected an honorary fellow of the Royal Society of Tropical Medicine and Hygiene, and has been invited to give the first Chadwick lecture in London during the present month.

DR. WILDER DWIGHT BANCROFT, professor of physical chemistry at Cornell University, has been appointed visiting professor of chemistry at Bowdoin College on the Tallman Foundation.

PROFESSOR G. A. MILLER, of the University of Illinois, was made an honorary life member of the Mathematical Association of America during its recent meeting at Duke University.

PRESIDENT ISAIAH BOWMAN, of the Johns Hopkins University, was granted the Distinguished Service Award of the National Council of Geography Teachers at the Syracuse meeting on December 30, in recognition of his contributions to geographic education.

DR. EDGAR ANDERSON, professor of botany at Washington University, St. Louis, and geneticist at the Missouri Botanical Garden, has been awarded the Order of the Yugoslavian Crown in recognition of his study of Balkan plants.

AT a ceremony at the University of Michigan, presided over by President Alexander G. Ruthven, on September 28, a portrait of Dr. Albert M. Barrett, director of the Psychopathic Hospital, was presented to the university by friends, alumni and associates of Dr. Barrett. It is the work of John Koch, of Ann Arbor.

THE twenty-fifth anniversary of the appointment of Professor S. O. Mast to the faculty of the Johns Hopkins University was celebrated at a dinner given in his honor at the Morton Hotel in Atlantic City on December 29. About forty of his present and former students and several of his colleagues and old college friends were present, as well as Mrs. Mast and two of their daughters. An oil portrait of Dr. Mast done by Hans Schlereth, of Washington, was unveiled. In addition, a bound volume of testimonial letters and a

handsome three-volume set of his 130 reprints were presented to him. Dr. H. S. Jennings acted as toastmaster, and toasts were given by Drs. C. Ladd Prosser, C. E. Bills, S. W. Geiser, W. N. Hess, O. S. Reimold, L. M. Bertholf, J. P. Visscher and W. L. Dolley, Jr.

AT the annual meeting of the Genetics Society of America held at Atlantic City officers for 1937 were elected as follows: E. M. East, Harvard University, *president*, and L. J. Cole, University of Wisconsin, *vice-president*. The secretary of the society is M. Demerec, Carnegie Institution of Washington, Cold Spring Harbor, New York.

THE Botanical Society of America, at its thirty-first annual meeting held at Atlantic City from December 29 to 31, elected the following officers for the ensuing year: *President*, Edmund W. Sinnott, Columbia University; *Vice-president*, Loren C. Petry, Cornell University; *Secretary*, George S. Avery, Jr., Connecticut College. Chairmen of the sections of the society elected or announced at this meeting are: *General*, E. N. Transeau, the Ohio State University; *Physiological*, E. F. Hopkins, Cornell University; *Systematic*, T. G. Yuncker, De Pauw University. A new section for paleobotany was organized, with the following officers: *Chairman*, A. C. Noé, University of Chicago; *Secretary*, W. C. Darrah, Harvard University. At the same meeting the following botanists were elected to corresponding membership: Dr. N. I. Vavilov, director of the State Institute for Experimental Agronomy, Leningrad; Dr. Agnes Arber, sometime fellow of Newnham College, Cambridge, England, and Dr. Lorenzo R. Parodi, professor of botany in the University of Buenos Aires.

THE American Microscopical Society held its fifty-fifth annual meeting at Atlantic City on December 30. The following officers were elected for 1937: *President*, Dr. W. W. Cort; *first vice-president*, Dr. O. E. Jennings; *second vice-president*, Dr. J. W. Scott; *secretary* (3 years), Dr. J. E. Ackert; *elective member of executive committee*, Dr. A. B. Dawson (3 years). Dr. Henry B. Ward, Dr. H. N. Lyon and William E. Dresher, who have served the society for fifty years, were elected to honorary membership. Dr. J. E. Ackert and Dr. A. M. Chickering were named to represent the society on the council of the American Association.

THE following members of the Indiana Academy of Science have been appointed divisional chairmen for the year 1937: *Archeology*, Glenn A. Black, Indianapolis; *Botany*, C. L. Porter, Purdue University; *Chemistry*, Paul D. Wilkinson, Indiana State Teachers College; *Bacteriology*, H. M. Powell, Eli Lilly Company, Indianapolis; *Geology and Geography*, Leroy Perkins, Indiana State Teachers College; *Mathematics*, C. K. Robbins, Purdue University; *Physics*, Leslie I. Steinbach, Central Normal College, Danville; *Psychology*, P. R. Hightower, Butler University; *Zoology*, W. P. Allyn, Indiana State Teachers College. Chairmen of the following standing committees have also been appointed: *Archeological Survey*, E. Y. Guernsey, Bedford; *Biological Survey*, B. E. Montgomery, Purdue University; *Library*, J. E. Potzger, Butler University; *Program*, Edward Kintner, Manchester College; *Publication of Proceedings*, J. J. Davis, Purdue University; *Relation of Academy to State*, F. N. Wallace, Department of Conservation. Dr. H. E. Enders, dean of the School of Science, Purdue University, has been selected as representative for the academy on the Council of the American Association for the Advancement of Science and as chairman of the Junior Academy of Science.

THE corporation of the Massachusetts Institute of Technology on January 7 elected, under a change in the corporation by-laws, five special-term members. The Technology Alumni Association nominates three term-members annually. The new members are: Edmund C. Mayo, president of the Gorham Manufacturing Company, Providence, R. I.; Gordon S. Rentschler, president of the National City Bank of New York; Ralph E. Flanders, president of the Jones and Lamson Machine Company, Springfield, Vt.; Frank D. Comerford, president of the Edison Electric Illuminating Company, Boston, Mass., and Halfdan Lee, president of the Eastern Gas and Fuel Associates.

PROFESSOR C. L. METCALF, head of the department of entomology at the University of Illinois, has been appointed chairman of the division of biological sciences, composed of the departments of bacteriology, botany, entomology, physiology, psychology and zoology.

ASSISTANT PROFESSOR W. D. BATEN, of the University of Michigan, has been appointed associate professor of mathematical statistics and research associate in statistics at Michigan State College. His work will include that of statistical adviser in the State College Experiment Station.

PREVIOUS awards from the Elizabeth Thompson Science Fund were reported in SCIENCE on November 29, 1935, and earlier. Since the last report the following awards have been made. At the meeting of April 8 $200 was awarded to Kurt G. Stern, Yale University, for an investigation of the chemical constituents of the enzyme catalase; $60 to Arthur Jacot, Appalachian Forest Experimental Station, Asheville, N. C., for duplicating Thomas Say's collection of mites and determining what were his species; $200 to Dr. H. S

Jennings, of the Johns Hopkins University, to be administered in behalf of Dr. T. T. Chen's studies on the mechanism of heredity in unicellular organisms; $400 to Walter C. Michels and A. L. Patterson, Bryn Mawr College, for studies of the effects of x-rays on cell division and for crystal analysis.

Dr. N. H. Darton has been retired from the U. S. Geological Survey, after serving as geologist for more than fifty years. He will continue scientific work at the survey and will also engage in private practice with headquarters in Washington.

Dr. William Thomas Calman retired from the British Museum (Natural History) on December 29, the day on which he reached the age of sixty-five years. Dr. Calman, who is president of the Linnean Society and secretary of the Ray Society, entered the museum in 1904, and has been keeper of the department of zoology since 1921. His special subject is the study of the crustacea. On the day of his retirement a number of his friends and colleagues presented him with a pencil portrait of himself, drawn by W. T. Monnington. The retirement on account of ill health is also announced of Guy Coburn Robson, deputy keeper in charge of mollusca in the department of zoology.

Albert Pérard, who has been connected with the International Bureau of Weights and Measures since 1905, since 1931 as assistant director, has been appointed director. He succeeds Dr. Charles-Edouard Guillaume, who has retired after serving in the bureau for fifty-three years and as its director since 1915. The title of "honorary director" has been bestowed upon Dr. Guillaume.

The British Medical Journal records the following retirements and new appointments: Professor Sobernheim, director of the Institute for Hygiene and Bacteriology at Berne, has retired and been succeeded by Dr. Kurt Hallauer, of Basle. Dr. Marinescu, professor of neurology at Bucarest, has been succeeded, on reaching the age limit, by Dr. Paulian, director of the Central Neurological and Psychiatric Hospital. Professor C. Kronacher, director of the Institute for Animal Breeding and Domestic Animal Genetics of the University of Berlin, has been given charge of the German Society for Animal Psychology, an institution created earlier this year. Dr. Leopold Arzt, professor of dermatology at Vienna, has been elected rector of the university; Dr. André Trèves has been elected president of the Paris Surgical Society.

Dr. Wilfred H. Osgood, chief curator of zoology at the Field Museum of Natural History, sailed on January 9 from Vancouver for French Indo-China to collect birds and mammals for the museum. The chief objectives of his expedition are specimens for a proposed habitat group of gibbons and for a group of Argus pheasants. Dr. Osgood will also collect a wide variety of small mammals, birds, reptiles, etc. He will remain in French Indo-China and neighboring territory for four months or more, working principally in the southern regions.

Professor Ellinor H. Behre, of the Louisiana State University, is taking the place during the first semester of Dr. Elizabeth Adams, of the department of zoology at Mount Holyoke College, who is absent on leave. Dr. Adams is visiting laboratories where endocrine investigations are in progress, including McGill University, the University of Buffalo, the University of Chicago, the Mayo Clinic, the University of Iowa and the Johns Hopkins University.

Visitors to the College of Natural Science of Yenching University, Peiping, China, during the autumn of 1936 included: Dr. O. R. McCoy, professor of parasitology at the School of Medicine and Dentistry of the University of Rochester; Miss Carey D. Miller, of the Nutrition Laboratory of the University of Hawaii; Dr. A. C. Fraser, professor of plant breeding at Cornell University; Dr. P. G. Rahm, visiting professor of biology at Fu-Jen University, and Dr. B. Suzuki, professor of biological chemistry at Tokyo Imperial University.

Dr. George W. Corner, professor of anatomy in the University of Rochester, has returned from England. During his visit there he presented, in addition to the Vicary Lecture of the Royal College of Surgeons, a series of lectures on the ovarian hormones, under the auspices of the University of London and Guy's Hospital Medical School on December 7, 8, 9 and 11. He lectured also at the University of Manchester on December 14, and at the Edinburgh Obstetrical Society on December 15, on "The Corpus Luteum and its Hormone," and at the University of Edinburgh on December 15 on "The Ovarian Hormones and the Menstrual Cycle."

Dr. R. Schoenheimer, assistant professor of biological chemistry of the College of Physicians and Surgeons, Columbia University, will deliver the fourth Harvey Society Lecture of the current series at the New York Academy of Medicine on January 21. He will speak on "The Investigation of Intermediary Metabolism with the Aid of Heavy Hydrogen."

The sixty-sixth annual meeting of the American Public Health Association will be held in New York City from October 5 to 8. The last annual meeting took place in New Orleans last October. It attracted an attendance of 1,650 health authorities representing forty-five states, Canada, Cuba, Mexico and nine other

foreign countries. The National Organization for Public Health Nursing will meet with the association in 1937 for the first time. This organization will, it is expected, add another thousand members to the registration lists. The following related societies will meet with the association as usual: The American Association of School Physicians; International Society of Medical Health Officers; Conference of State Sanitary Engineers; Conference of State Laboratory Directors; Association of Women in Public Health and Delta Omega. Dr. Reginald M. Atwater is the executive secretary of the association, and the headquarters offices are at 50 West 50th Street, New York, N. Y.

DISCUSSION

THE DISTRIBUTION OF BLACK WIDOW SPIDERS

IN a recent article by D. C. Lowrie[1] the geographic distribution of the black widow spider, *Latrodectus mactans* (Fabr.), is discussed. The author adds Indiana to the list of states from which the spider is known and refers to the records given two weeks previously by L. H. Townsend[2] for Illinois and Oregon as though they were the first for those states. Moreover, he makes a statement to the effect that it has not yet been recorded from the following states: Minnesota, Iowa, Virginia, Delaware, New Jersey, Connecticut, Rhode Island and Vermont. A few weeks later Jeffers[3] recorded the spider from Virginia. There is no doubt that it occurs in all these states, as it has been found in the regions bounding them. As a matter of fact, records for some of them have been available in entomological literature. For the benefit of those interested who may not have access to this literature I repeat them here.

For Virginia the spider was first recorded by J. H. Emerton[4] in 1875, from Hog Island. It has also been recorded as abundant in the Norfolk area by L. D. Anderson and H. G. Walker,[5] and from various localities by C. R. Willey.[6] Moreover, Dr. Bogen[7] cites cases of arachnidism from this state and gives three medical references. In a supplementary paper[8] two more references are added.

For Illinois the spider has been recorded by W. J. Spicer[9] from near Pittsfield, near Springfield and from Barry. For Oregon, by H. H. Stage[10] from Klamath Falls, and by D. C. Mote[11] from Roseburg and elsewhere.

For New Jersey the spider has been recorded by C. H. Hadley[12] from Moorestown, and by R. C. Casselbury[13] from near Ocean City. For Rhode Island a specimen from Cranston has been recorded by A. E. Stene.[14] Specimens have been collected in Connecticut at Killingworth on May 16, 1933, by Dr. A. Petrunkevitch; at North Plains on October 28, 1934, by Dr. S. C. Ball; at Norwichtown on June 25, 1935, by A. Latham, and at Leetes Island on September 29, 1935, by D. S. Riggs. These have all been recorded by Dr. W. E. Britton.[15]

In addition to the above the following records are published for the first time. In Connecticut a specimen was taken by P. G. Howes at Stamford in 1912; by V. R. Short at Westbrook on June 8, 1935; by Mrs. W. Harrington at Woodbridge on October 7, 1936; by Mrs. I. J. Longo in Bridgeport on September 15, 1936, and two were found by D. S. Riggs in the nest of a mud dauber wasp at Cheshire on August 19, 1936. In Vermont specimens were collected in June, 1935, by Miss E. B. Bryant at Brandon and by C. H. Paige at Woodstock.

B. J. KASTON

CONNECTICUT AGRICULTURAL EXPERIMENT STATION,
NEW HAVEN

[1] SCIENCE, 84: 437, November 13, 1936.
[2] *Ibid.*, 84: 392–393, October 30, 1936.
[3] *Ibid.*, 84: 533–534, December 11, 1936.
[4] Occ. Papers Boston Soc. Nat. Hist., 2: 153, 1875. (In reprint of N. M. Hentz's "Spiders of the United States.")
[5] *Insect Pest Surv. Bul.*, 12: 404, November, 1932.
[6] *Ibid.*, 14: 296, November, 1934.
[7] *Arch. Int. Med.*, 38: 623–632, November, 1926.
[8] *Ann. Int. Med.*, 6: 375–388, September, 1932.
[9] *Insect Pest Surv. Bul.*, 15: 419, November, 1935.
[10] *Ibid.*, 14: 164, July, 1934.
[11] *Ibid.*, 14: 209, August, 1934.
[12] *Ibid.*, 15: 389, October, 1935.
[13] *Entom. News*, 46: 260–261, December, 1935.
[14] *Insect Pest Surv. Bul.*, 16: 306, August, 1936.
[15] *Conn. Agr. Exp. Sta. Bul.*, 383: 350. April, 1936.

THE BLACK WIDOW SPIDER IN VIRGINIA

As G. W. Jeffers states in SCIENCE for December 11, 1936, it is rather surprising that the black widow spider *Latrodectus mactans* has not been recorded officially in Virginia, although this is the case, according to D. C. Lowrie in SCIENCE for November 11, 1936. Jeffers finds it fairly common at Farmville, Va., and concludes that it probably occurs elsewhere in the state.

The writer has found it under boards at Arlington Farm, Va., near Washington, D. C., in the Alleghenies at Camp Todd, Augusta County, Va., at 1,000 feet and at the foot of Walker Mountain near Deerfield, Augusta County, Va., all within the last two years.

Many years ago the writer found it very abundant at Thompsons Mills near Hoschton in northern Georgia. At this time little mention was made of this spider and the writer strongly doubted the venomous nature of its bite. Several attempts were made to test its bite between the fingers, but he could not get it to use its fangs.

On collecting trips as many as five or six were carried alive in the closed hand on several occasions with no effort on its part to bite.

These spiders seek concealment usually beneath boards, logs, bark and stones. They are easily reared from the cocoons, and if one is dropped on the floor of a box with a female she proceeds to suspend it in a web and watch over it. The process is repeated if others are dropped about, showing a rather marked solicitude for the nest.

The writer has liberated swarms of the young in an old woodpile near his garden, with no fear of being bitten. Much has been written within recent years about the evil ways of this spider, but there is little reason to fear its attacks and no reason to wish that it could be exterminated. In truth the writer has no desire to exterminate unconditionally even the rattlesnake or copperhead in its wildest haunts, so marvelously has nature designed the rattlesnake more especially, and in the New World alone. The true naturalist feels no cynicism because nature has placed these in our midst and would not rejoice at their complete extermination.

H. A. ALLARD

WASHINGTON, D. C.

THE SIMILARITY OF ACTION OF MALE HORMONES AND ADRENAL EXTRACTS ON THE FEMALE BITTERLING

IN a recent issue of SCIENCE the observation was reported by Barnes, Kanter and Klawans[1] that crude ether extracts of adrenal cortex can initiate the lengthening of the ovipositor of the female bitterling. It was also stated that crystalline androsterone did not produce a positive reaction with these fish. Both of these observations would seem to cast doubt on our contention that the phenomenon in question is evoked by the male hormone.[2]

We wish to point out that such a conclusion is not necessarily true. In the first place, the failure to get a positive reaction with crystalline androsterone in one experiment using two fish is hardly convincing. We have performed many experiments with crystalline synthetic androsterone and have seen a number of positive reactions.[3] Positive reactions are usually, although not always, obtained when the optimum dose and a suitable menstruum are employed. Synthetic testosterone[4] also has been found effective.

But how does the action of adrenal cortical extract harmonize with the male hormone hypothesis? Reichstein[5] has obtained a substance from the adrenal cortex which is capable of stimulating comb growth in the capon, *i.e.*, a compound resembling androsterone physiologically. Mason, Myers and Kendall[6] have oxidized a cortical substance, similar to cortin, into a ketone which also has the stimulating effect on the capon's comb. It thus appears that adrenal cortex contains one or more substances resembling androsterone. These, from our experience, would be expected to have the effect on the female bitterling which Barnes, Kanter and Klawans have found.

The relationship of the adrenals to secondary male characteristics has long been recognized. It is to be hoped that the interesting facts referred to in this note will lead to more definite knowledge in this field.

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[1] B. O. Barnes, A. E. Kanter and A. H. Klawans, SCIENCE, 84: 310, 1936.

[2] I. S. Kleiner, A. I. Weisman and D. I. Mishkind, *Jour. Am. Med. Asn.*, 106: 1643, 1936.

[3] I. S. Kleiner, A. I. Weisman and D. I. Mishkind, *Proc. Soc. Exp. Biol. and Med.*, 35: 344, 1936.

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PARTHENOGENESIS IN THE GRASSES

IN the November 13th issue of SCIENCE the article on the "Possibility of Parthenogenesis in Grass" suggests that it may be the first report of parthenogenesis in the grasses. There are, however, at least three reported cases: The first by J. De Coulon, "Nardus stricta. Etude physiologique, anatomique et embryologique," *Mem. soc. Vaudiose sc. nat.*, 1: 245–332, 1923; the second by E. F. Gaines and H. C. Aase, "A Haploid Wheat Plant," *Amer. Jour. of Botany*, 13: 373–385, 1926; the third by Helge Stenar, "Parthenogenesis in der Gattung Calamogrostis," *Arkiv. für Botinik.*, 25: 1–8, 1 Taf., 2 fig., 1935.

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ALKALIZE, ALKALINIZE AND ALKALIFY

THREE words are recorded in standard English and American dictionaries to denote the operation of making a material alkaline. These words are "alkalize," "alkalinize" and "alkalify." The word "basify" appears too, but it is defined as meaning "to make into a base by chemical means," which is not equivalent to the other three. Although direct analogy would suggest "alkalinify" as the opposite of acidify, this word is not listed at all.

While acidulate and acidify are familiar enough, the nearly universal practice among chemical writers is to say "add alkali until alkaline" or something equivalent, rather than use the less cumbersome words, alkalize, alkalify or alkalinize. Perhaps if it were known

that these words are perfectly acceptable, more writers would employ them. This would make for greater simplicity and often for greater clarity in setting down laboratory directions.

So unfamiliar are alkalify, alkalinize and alkalize that many instructors have made a habit of correcting students of elementary chemistry who have used them. Yet "alkalize" has had recognized standing since 1749.

This year a greater number of students than average have sought to use "alkalize" in place of more round-about expressions of the same idea. Probably their practice was inspired by the advertisements of a certain laxative mixture, where the word is used rather loosely. But whatever the source of the stimulus, there is no reason why alkalize, alkalinize or alkalify should not have wider usage. Rather than reprove the students for using these words, we might well follow their example.

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SPECIAL CORRESPONDENCE

FOURTH ANNUAL TRI-STATE (ILLINOIS, IOWA, WISCONSIN) GEOLOGICAL FIELD CONFERENCE

GEOLOGISTS and students of geology in the three above-mentioned states participated in the annual tri-state field conference on October 31 and November 1. The conference was held this year in Calhoun and Jersey counties in central western Illinois. It was conducted by A. H. Sutton, University of Illinois, assisted by J. Marvin Weller, Illinois State Geological Survey.

The conference was attended by 117 persons, who traveled in 35 cars. Geologists from eleven universities, colleges and state surveys of the three states and representatives of six oil companies operating in Illinois were present. Invited guests of the conference included six persons from Washington University, St. Louis, Mo., one from Oklahoma A. and M. College and the manager of the Alton, Ill., *Telegraph*. The geology of the stops was described in a mimeographed log and a blue-print map, furnished each participant at the beginning of the conference. In addition each car was supplied with quadrangle topographic maps of the area visited.

The conference began at Hardin, Calhoun County, at 9 A.M. on Saturday. The first day's trip included eight stops in Calhoun County. The stratigraphic section studied during the day is summarized below: *Mississippian:* St. Louis, Spergen (Salem), Warsaw, Keokuk, Burlington, Sedalia (Fern Glen), Chouteau, Hannibal, Louisiana, Saverton. *Devonian:* Cedar Valley. *Silurian:* Joliet, Kankakee, Edgewood. *Ordovician:* Maquoketa, Kimmswick, Decorah, Plattin, Joachim, St. Peter.

Good exposures of all these formations were visited for examination and fossil collecting. Contacts between most adjacent formations were observed. The Cap-au-Gres faulted monocline was studied and discussed. G. E. Ekblaw, Illinois State Geological Survey, explained the origin of the terraces along Illinois River and gave a brief summary of the Pleistocene and recent history of the area. W. H. Twenhofel, University of Wisconsin, and J. E. Lamar, Illinois State Geological Survey, discussed problems of the St. Peter sandstone, comparing the formation in this area with that in the northern portion of the Mississippi Valley.

The annual dinner and general meeting was held at the Stratford Hotel in Alton, Ill., on Saturday night and was attended by 103 persons. No formal papers were presented, but geologic problems of the area were discussed. Dr. Ekblaw presented a more detailed summary of the geologic history than had been given earlier in the day.

On Sunday, November 1, the trip covered portions of Jersey County. Several of the stratigraphic units which had been examined the previous day were seen again, and the Cap-au-Gres structure was studied at more localities. The conference closed at noon on Sunday at an exposure of Pleistocene varved lake deposits which were made in a pond adjacent to the margin of the Illinoian Ice.

The conference will be held next year in Wisconsin under the leadership of Professor F. T. Thwaites, of the University of Wisconsin.

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SPECIAL ARTICLES

BUILT-UP FILMS OF PROTEINS AND THEIR PROPERTIES

MANY proteins can exist in water as large spherical molecules, but they can also spread on water surfaces, giving elastic solid monomolecular films having great two-dimensional compressibility. The present paper describes experiments made to determine whether methods[1, 2] developed in this laboratory for stud…

[1] I. Langmuir, *Jour. Franklin Inst.*, 218: 143, 1934.

[2] Katharine B. Blodgett, *Jour. Am. Chem. Soc.*, …: 1007, 1935.

of monolayers of higher fatty acids are applicable to monolayers of proteins.

We have been able to transfer monolayers of protein from a water surface onto solid surfaces, where their thickness can be measured by optical methods and many new properties can be observed.

As a solid substrate upon which to build up such films we have found it preferable to use a surface already covered with a number of layers of barium stearate obtained by the method described by Dr. Blodgett.[2] For example, we use a plate about the size of a microscope slide consisting of highly polished chromium plated brass. If 37 to 47 layers of barium stearate are placed upon this plate, the interference colors observed with polarized light at large angles of incidence are so sensitive to changes in thickness of the film that an increment of 3×10^{-8} cm produces noticeable change of color. A single monolayer of protein thus produces a very striking color change. A further development of Dr. Blodgett's technique, using monochromatic light and a photocell to determine the relative reflectivities of adjacent steps, should make it possible to measure variations in thickness much less than 10^{-8} cm.

A monomolecular film of protein may be transferred to a solid surface prepared in this way as follows: The surface of distilled water in a tray is cleaned by scraping with a barrier. A narrow strip of paper is placed upon the water near one end of the tray. A platinum wire, to which a few particles of protein, such as egg albumin or pepsin, are attached, is made to touch the surface of the water and the monomolecular film spreads out from these particles, pushing ahead of it the floating paper strip. A small drop of purified oleic acid is then applied to the water on the other side of the paper strip, so that a surface pressure of about 30 dynes per cm acts upon the protein film. The paper strip indicates the boundary between the protein film and the oleic acid film.

MONOFILMS

A single monolayer of protein may now be transferred to the prepared plate in two different ways. In the first method, which we shall denote as Method A, the plate, held in a vertical plane, is lowered into water. The movement of the paper barrier toward the plate proves that the protein film is being transferred to the plate. The plate is then kept immersed in the water while the protein film is removed from the surface by scraping and by blowing any residual film to the opposite end of the tray, where it may be confined behind a barrier. The plate, when raised out of the water, comes out wet, whereas if the protein film had not been placed upon the plate, the plate would have shed water when it was withdrawn. After allowing the surface film of water to evaporate, examination of the plate with polarized light at angles near grazing incidence shows that the part of the plate to which the protein film has been applied differs markedly in color from the original stearate film. Comparing the color with that of stearate films having a known series of steps, it is seen that the thickness of a film of egg albumin obtained under these conditions is about 20×10^{-8} cm.

The second method of applying the protein film, which we shall call Method B, consists in lowering the prepared plate vertically through a clean water surface, then applying the protein film as before and raising the plate out through this film. The motion of the paper strip again shows that a protein film is transferred to the plate. However, since the plate comes out wet and at first shows no interference colors, it is evident that the protein film is not yet in contact with the plate but lies on a water film several microns in thickness. As the water dries, the protein film becomes attached to the substrate. This film has about the same thickness as that obtained by Method A.

It is remarkable that, although the prepared plate (without a protein film), lowered into clean water and withdrawn, comes out dry, it is covered by a thick water layer if the plate is withdrawn from water upon which there is a protein film. If before drying the slide, it is lowered into the water, the movement of the paper barrier shows that the protein film goes back on the water surface, notwithstanding the 30-dyne pressure exerted by the oleic acid. These phenomena at first suggest that the protein film acts through the water film, a distance of several microns, upon the stearate film on the plate and so modifies it that it remains wet. A closer examination of the process by which a stearate film sheds water proves, however, that this depends upon the presence of a line of contact between the water-air, the water-stearate and the air-stearate interfaces. This one-dimensional contact line is the seat of the phenomenon. The forces acting along this contact line which are enormously more intense than any that can be exerted by gravity on a water film of a few microns' thickness produce a "zipper-like" effect in closing up the space available for the water film.

The action of the protein film by which it prevents the shedding of the water is thus to be interpreted as evidence that the work of adhesion between the protein film and the stearate film is not sufficient to give a sufficiently strong zipper action. The film of water is then about twice as thick as that occurring on a *clean* glass slide withdrawn from water, since the water film, descending only by gravity, is confined in the first case

between two stationary surfaces (the plate and the protein film), while in the case of the glass slide one surface of the water is free.

Multiple Films

We have found it possible to build up multiple protein films under certain conditions. To classify the types of film obtained let us use P to denote the plate, R for the hydrocarbon surface upon it (barium stearate layers with CH_3 radicals forming the surface), A and B for protein layers produced by Methods A and B, respectively. Thus, for example, PRAB denotes a prepared plate upon which there is an A film covered by a B film.

PRAB Films

By dipping a plate into water covered by a protein film, withdrawing it and drying the water film, two layers of protein can be transferred to the plate. The thickness indicated by the color change is approximately twice that of a single layer. With pure water as the liquid substrate we have not succeeded in repeating this process to build up films having the structure PRABAB. If a PRAB film is lowered into pure water or into water covered by a protein film, even under 30 dynes/cm pressure, the B film is ejected from the plate on to the water surface. This is evident from the motion of the paper strip. The loss of the B-layer from the plate when it is dipped into the water has also been proved by withdrawing the plate through a clean water surface, drying it and examining the color.

The addition of zinc chloride to the water (10 mg per liter) prevents the separation of the B from the A layer and makes it possible to continue adding AB layers indefinitely giving PRABABAB ... films. In this way 30 layers were built up without difficulty and the thickness as indicated by color increased in proportion. We believe that very accurate measurements of the thickness of the films can be made in this way. They should also be useful for study of structure by x-ray and electron diffraction.

PRBBB ... Films

Successive B films can be built up, even without adding zinc salts, by lowering the plate into clean water and withdrawing it through a protein film. The plate always comes out wet and the new layer must be dried on before the next one is applied. The dried-on B-films are not ejected from the plate either on immersing or withdrawing the plate.

PRBAB Films

After a single B film has been applied and dried, the plate takes up an A film if it is lowered through a protein film. When the plate is withdrawn through a clean water surface, a PRBA film is formed; if withdrawn, through a protein layer, PRBAB is formed. Further than this, we have not been able to go without adding zinc salts, since the last B film is ejected if the plate is lowered into water. We have not succeeded in producing PRAAA films, but with difficulty have obtained imperfect PRAA films.

All the types of films which we have been able to build using oleic acid pressure (30 dynes/cm) can be built without appreciably greater difficulty, using castor oil (about 15 dynes/cm) as piston oil.

Properties of the Protein Films on Solids

The foregoing observations lead to the conclusion that there are some essential differences between A layers and B layers. For example, B layers which lie upon A layers are ejected on to the water surface on immersing the plate in water (in absence of zinc salts); whereas B layers upon B layers are not ejected. The methods used to form the layers indicate that A layers are turned upside down (inverted) in their formation, whereas the B layers are not inverted. Thus the outer surfaces of the A and B layers should be hydrophilic and hydrophobic, respectively.

The adhesion of B or A layers to a PR substrate is such that dipping into water does not cause the removal of the layer.

The fact that A and B layers preserve their identity after immersing in water indicates that they can not readily turn over. This supports the theory that they consist of a two-dimensional network rather than polypeptide chains.

The outer surfaces of both A and B layers are wettable by water and by hydrocarbons such as hexadecane, petrolatum, benzene and hexane, and show no striking differences in contact angles. If either A or B layers are partly covered by petrolatum and then a drop of water is placed on an adjacent place on the layer, it can be observed on tilting the plate that the water displaces the hydrocarbon. This action is considerably more marked with an A film than with a B film and gives some evidence for the greater hydrophilic character of A.

The most striking evidence that the outer surface of A and the inner surface of B are predominantly hydrophilic is furnished by the ejection on to the water of a B layer, which rests upon an A layer. This action is undoubtedly caused by the affinity of this hydrophilic interface for water.

Stearate Films Built upon Protein Films

When a prepared plate PR, partly covered by an A or B film, is lowered into water containing Ba salts covered by a stearic acid film, under 30 dynes pre-

sure, it is seen that the water rises on the PRA or PRB film (contact angle of about 40°), whereas it is strongly depressed on the PR portions (contact angle far greater than 90°). When the plate is withdrawn through the stearate film on the water, the PR portions come out dry (with two additional stearate layers), while the PRA or PRB portions are wet. After drying, the color indicates that two stearate layers have been added on top of the protein film. If the plate is again lowered and raised through the stearate film on the water all portions of the plate come out dry and two more stearate layers have been added to the whole plate. In this way it has been possible to sandwich any number of single protein layers between layers consisting of even numbers of stearate monolayers.

PERMEABILITY OF PROTEIN FILMS

The composition of built-up barium stearate films depends upon the pH of water.[3] With strongly acid water, pH = 3, the films are nearly pure stearic acid, whereas with pH = 9 they are nearly pure neutral barium stearate. At pH = 6.5 the barium content is about half of that in barium stearate.

Dr. Blodgett has found that the films of neutral barium stearate remain unchanged in color after immersing in benzene, but when the barium content is decreased by using a lower value of pH during formation of the film, the free stearic acid can be rapidly dissolved out of the film by benzene. Her measurements of refractive index of such films have shown that the change of color produced by immersing a part of the film in benzene is due to a change of refractive index of the film and not due to a change of thickness. Stearate films, from which free stearic acid has been removed, may be called *skeleton films,* since the barium stearate lattice remains unchanged, while the molecules of free stearic acid are removed, very much as the water molecules in a zeolite crystal can be removed without altering the silicate lattice.

If a drop of a liquid hydrocarbon such as petrolatum or hexadecane is placed upon a skeleton film, it shows a lower contact angle than upon a film of neutral barium stearate; but the drop can still be made to move about on the plate without wetting it. The drop, however, leaves behind it a trail of the same color as the portions of the stearate film which have not been dipped into benzene. Thus the hydrocarbon immediately returns into the holes left by the removal of stearic acid and restores the refractive index to its original value, 1.49 (values as low as 1.25 may be obtained with skeleton films).

Vapors of octane and decane brought into contact with skeleton films also cause the refractive index to rise to 1.49, but when the source of vapor is removed, evaporation of the hydrocarbon causes a gradual return to the original lower value characteristic of the skeleton. A large number of non-volatile organic substances in benzene solution can be introduced into the holes of a skeleton film by dipping the film into such a benzene solution, and the extent to which the film takes up these substances can be quickly and accurately determined by the color changes. Such films thus constitute molecular sieves which may be used to determine the sizes, shapes and surface affinities of organic molecules.

It has also been possible to place a few layers of neutral stearate upon fifty layers of acid stearate and to measure the rate at which benzene removes free stearic acid through the insoluble neutral layers. In a couple of hours practically all the free stearic acid (equivalent to 15 layers) can be removed from 50 layers of acid stearate through 20 layers of neutral stearate, whereas without the addition of the neutral layers the removal would have been nearly complete in 1 or 2 minutes.

The foregoing technique may be applied in several ways to study the permeability of protein monolayers to various organic substances.

Method C: A protein film (A or B) may be applied to a prepared plate of acid stearate and if desired covered by 2 or 4 additional layers of acid stearate. Then the plate is immersed in benzene for definite time intervals, after each of which the color is observed. Petrolatum, the vapor of a volatile hydrocarbon or a benzene solution of an organic substance, is then applied to the plate and the color changes are noted. The advantage of covering the protein film by additional stearate layers is that a hydrocarbon liquid does not wet the film. Without such additional layers, the hydrocarbon wets the protein layer and forms such a thick layer of liquid that interference colors are not obtained.

Method D: A protein film may be applied directly upon a skeleton film and the taking-up of hydrocarbons or other substances may be studied.

In measurements made by Methods C and D with monolayers of egg albumin we have found that protein films are very much more impermeable (of the order of 100-fold) to benzene, stearic acid and the lower aliphatic hydrocarbons than are equally thick films of neutral barium stearate. Protein films applied under 30 dynes/cm are more impenetrable (of the order of 10-fold) than similar films applied under 15 dynes/cm. Protein films seem to be almost wholly impervious to petrolatum molecules, for a skeleton film $PR_{45}AR_2$, over which a petrolatum drop has been made to pass,

[3] I. Langmuir and V. J. Schaefer, *Jour. Am. Chem. Soc.*, 58: 284, 1936.

undergoes no greater change of color than would be expected from the hydrocarbon that enters the two upper layers R_2. Without the A the color returns to that of the unskeletonized film.

Several observations on the rate of removal of stearic acid by benzene have given indications that an A film is somewhat more impermeable than a B film.

EFFECT OF SURFACE PRESSURE ON THE THICKNESS OF PROTEIN FILMS

The area covered by a film of egg albumin on water decreases to one half when the surface pressure is raised from 15 to 30 dynes/cm. The thickness observed with $PR_{41}B_5$ applied under 15 dynes pressure agrees well with the color of $PR_{41}B_4$ applied under 30 dynes/cm pressure. Thus the thickness of the two kinds of films transferred to the solid differs in the ratio 1.0 to 1.25, while on the water surface the ratio is 1 to 2.

This difference suggests that strong forces of adhesion act upon the protein film on the solid to hold it flat so that the spacing is determined by the C-C and C-N linkages. On the other hand, the presence of the many hydrophilic groups in the protein molecules enables the lower surface of the protein monofilm on water to become wavy and so get into better contact with water. This waviness would account for the marked compressibility on water and the relatively smaller compressibility when forced to lie flat on a solid surface.

We have made some preliminary experiments to devise methods for studying protein films at the interface between water and hydrocarbon. A piece of egg albumin attached to a platinum wire was brought into contact with the interface between a lens of petrolatum and the underlying water. The lens was rapidly deformed in shape and in places made so thin that interference colors were obtained. The duplex films[2,4] thus produced are remarkably stable, as there is no tendency for the petrolatum to peel back, leaving a monolayer of protein on the water. The method just described is apparently not suitable for the formation of a uniform duplex film. A substance such as egg albumin, however, can be introduced as a water solution under the petrolatum. As the spherical molecules come into contact with the film, they appear to unfold into monolayers at the interface. Duplex films produced in this or other ways should afford a useful way of studying interfacial protein films. The preliminary observations show that such films are elastic solids of high compressibility, very much like protein films on water. With stearic acid, films at a water-air and an oil-water interface are very different, usually being condensed in the first case and gaseous in the second.

[4] I. Langmuir, *Jour. Chem. Phys.*, 1: 756, 1933.

Most of the experiments described in this paper have been carried out with egg-albumin on distilled water brought to about pH 7 by the addition of a trace of ammonia. In some cases we have changed the pH to 3 and to 10 but have not observed any marked differences in behavior. A few experiments with pepsin and insulin have shown similar behavior. Undoubtedly by the application of these methods, quantitative differences will be found between the proteins which form monolayers on water.

The addition of formaldehyde to the water under a protein film has been found to decrease greatly the compressibility of these films, presumably by forming new cross-linkages which prevent the waviness of the lower surface or hold the waves more nearly rigid.

The properties of proteins shown by our experiments seem to be in accord with the view that the protein monolayer is a two-dimensional network held together by strong elastic springs and are not in accord with a structure consisting of polypeptide chains.

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